

Letter

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Is there a positive association between mammographic density and bone mineral density? Authors' responseCarolyn Crandall¹, Shana Palla², Beth A Reboussin², Giske Ursin^{3,4} and Gail Greendale⁵¹Division of General Internal Medicine, David Geffen School of Medicine at University of California, Los Angeles, CA, USA²Department of Public Health Sciences, Wake Forest University School of Medicine, Winston-Salem, NC, USA³Department of Preventive Medicine, USC Norris Comprehensive Cancer Center, Los Angeles, CA, USA⁴Department of Nutrition, University of Oslo, Oslo, Norway⁵Department of Geriatrics, David Geffen School of Medicine at University of California, Los Angeles, CA, USACorresponding author: Carolyn Crandall, cgrundall@mednet.ucla.edu

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See related research article by Crandall *et al.* in issue 7.6 <http://breast-cancer-research.com/content/7/6/R922>and related letter by Dite *et al.* in issue 8.1 <http://breast-cancer-research.com/content/8/1/401>

We read with interest the issues raised by Dite and coworkers [1] in their response to our recent research article [2], and we are pleased to address them. In their letter, Dite and colleagues present previously unpublished results that reveal no association between bone mineral density and mammographic density in female twins. Relevant information about the characteristics of the study participants, the methods of measuring mammographic density, and the timing of the mammograms in relation to the bone density assessment are not supplied. Differences in study samples, methods, and sample size could account for the difference between their results and ours.

Dite and coworkers [1] posit several explanations for the apparent discrepancy between their study and ours. First, they suggest that our results were an artefact of our performance of multiple analyses. In fact, we performed only three regression analyses, and these analyses were based on clearly stipulated *a priori* hypotheses. We believe that a strict Bonferroni adjustment for multiple testing would have been overly conservative.

Second, they argue that we might have been misled by outliers or influential points. However, we performed outlier analyses using the dfBeta approach [3], which confirmed that our results were not unduly influenced by extreme data points. It is impossible to rule out chance as a possible explanation for our results, but with a few exceptions this criticism applies to all studies.

Finally, Dite and colleagues state that we might have assumed effect modification without testing for it. We tested for effect

modification between recent use of hormone therapy and lumbar bone mineral density by adding an interaction term to the linear regression model that included age, body mass index, smoking, lumbar bone mineral density, and recent hormone therapy as covariates, and mammographic density as the outcome. The *P* value for the lumbar bone mineral density-recent hormone therapy interaction term was 0.0583 at the lumbar spine. Similar results were apparent at the hip (*P* = 0.094 for the interaction term). On this basis, we conclude that recent hormone therapy modifies the association between bone mineral density and mammographic density. We stand by our results and hope to elucidate what biologic factors may be responsible for the association between mammographic density and bone mineral density in our study.

Competing interests

The authors declare that they have no competing interests.

References

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